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Ocean Science Discussions

Interactive comment on "Interannual coherent variability of SSTA and SSHA in the Tropical Indian Ocean" *by* J. Q. Feng

Anonymous Referee #1

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This study uses singular value decomposition on SSTA and SSHA and extended associated pattern analysis to examine coherent variations in both parameters in the Indian Ocean. This paper does not really add anything to the literature on the coherency of SST and SSH in the Indian Ocean nor the role of Rossby waves, other than applying new tools to the data and extending the time-series more. The patterns of the mode 1 are quite similar to the patterns Chambers et al (1999) and Tourre and White (1997) found in the region, using EOFs of the SST and SSH separately. Other authors (Chambers et al., 1999; White, 2000; Murtugudde et al., 2000) and a paper not even referenced (Huang and Kinter III, JGR, 2002) do a much better job describing the mechanisms and propagation of the Rossby waves. As this paper stands, I cannot recommend it for publication.

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I suggest rejecting the paper outright. However, if the editor decides for a major revision instead, then the authors need to comment and discuss how their results are different from the previous studies, and in what ways they support the previous work. In particular, how are their tools better than the ones used in previous work? Can they actually show they are better, or do they merely project the data differently? I also became very confused by the discussion of the sandwich mode and the lag correlation analysis in Figure 5. This shows the lag correlation of SSHA 2nd mode and SSTA first mode. How about SSHA 1st mode and SSTA second mode? SSHA 1st and 2nd mode? I'm not sure what the point of this exercise is. Clearly, as the authors later discuss, this is a sign of a propagating signal (i.e., the Rossby wave which we have known about for nearly 20 years), so the results are not surprising. The authors have merely found Rossby wave signatures in their technique, which should not be shocking to anyone. Using the technique to discover some new processes of the Rossby waves in relation to the Dipole (or ENSO) signal is useful, but just to show that Rossby waves exist is not. I would suggest if the authors want to pursue this, they extend their techniques to winds as well as SST and SSH, since it is the wind variations that ultimately drive the Rossby waves (e.g., Chambers et al., 1999).

Moreover, there are numerous typos and grammar problems, far too many to document here. This paper needs a careful editing by a native English speaker before it can be published (or go back out for review).

Other Comments

1. References needed for the SSHA and SSTA data in Section 2.

2. Use Nino3.4 index, derived from different data set from the one used. Should recompute from NCEP SST data to be consistent.

3. Throughout, "technic" should be "technique"

4. References to Figures jump all over the place. For example, in Section 3.1, para-

OSD
9, C12–C14, 2012

graph 2, the author discusses Figure 1, then Figure 5c, then Figure 3a. Figures should be ordered as they are discussed. If the current Figure 5c is that important to the discussion, it should be Figure 2.

5. Figure 6. Why are SSTA extremes colored, but not SSHA. Please do SSHA as well so one can more easily follow Rossby waves.

6. References inconsistent in dates several times.

Interactive comment on Ocean Sci. Discuss., 9, 1, 2012.